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COMETS—A MATTER OF LIFE AND DEATH

Sir Fred Hoyle

The Text of the Milne Lecture, Delivered at Oxford, on 16 January 1978

I find it somewhat strange to be giving this first Milne lecture, since I was not fortunate enough to know Professor Milne very well. I was half-a-generation too young to be on anything like familiar terms with Milne, and I was from the wrong university to have come in contact with him as a student. Also, I came to astronomy rather definitely from a background of theoretical physics, whereas the tradition in those days among British astronomers lay much more in applied mathematics.

Doubtless, it was my connections with theoretical physics that caused me to take a different view of the astronomical problems of the 1940s than did the general body of British astronomers. From time to time the R.A.S. used to allow me a few minutes to state my views, and invariably on such occasions my remarks were followed by fusillades of objections, as you may readily verify from a glance at old copies of the Observatory Magazine. There were always just one or two people in the audience, however, whose comments were mild and helpful and invariably among those one or two would be Professor Milne.

Occasionally thereafter we would snatch a few minutes of personal conversation. as one might do at an R.A.S. tea or reception. On one such occasion, Milne asked me, rather shyly, if I had come in my work on anything to support his view that the mathematical boundary conditions at the surface of a star had an important controlling influence on the internal structure of the star. You may recall that in this contention Milne had been opposed by Eddington, and that the astronomical world in general had accorded Eddington the victory in the ensuing controversy.

It has always been a matter of regret to me that I had at that time to tell Milne that I had found nothing to support his position. Yet several years later, by the middle 1950s, Martin Schwarzschild, of the University of Princeton, and I had indeed found an important class of star for which Milne's contention was correct. For a while I thought of basing my present lecture on that work from the 1950s. It would perhaps have been nice to show how it came about that Milne was partially right after all, indeed that both parties to my old controversy had been partially right. But to have done so would have been to present a lecture on astronomical archaeology, for by now the situation has become widely understood, and the sweep of current research has changed. This being so, it eventually seemed to me that Milne himself would much have preferred me to give a lecture in the spirit of those wild old sessions at the R.A.S., a lecture with which most of you in this audience will quite likely disagree.

Let me come first of all to our Chairman himself. Our Chairman has strong views on the nature and origin of comets, so let me say that a comet for me will not necessarily be the same as a comet for him. By a "comet" I shall mean a planetesimal formed during the process of origin of the planets Uranus and Neptune. There is not too much to object to in this, I suppose, since it is largely a matter of definition. But I shall also require that a fraction of such planetesimals have been left over to the present day, and I shall require that a fraction (but not necessarily all) of the comets we observe are objects of this type. If our Chairman can accept such a position, at any rate for the short space of an hour, then my lecture may not be as "far out" (or should I say unpalatable) for him as it may possibly be for some others.

The first conventional position that I wish to challenge is that life originated here on the Earth. Twenty years ago, before it was known that organic molecule existed in large quantities within the interstellar gas clouds, this was not an unreasonable view. Today, one can see that it has little or nothing in its favour. It suffers from the overwhelming difficulty of having to begin without organic molecules, in contrast to an external theory which can *begin* with the kind of molecules shown in Table 1. These have all been detected in the interstellar clouds Very strikingly, the concentrations of the more complex molecules in this Table are found to be comparable with the concentrations of less complex ones, a circumstance which points to these observed molecules being break-up fragments from still larger organic molecules (for, of course, a gaseous synthesis from individual atoms at very low density would lead with increasing complexity to sharply falling concentrations).

Another indication of the presence in space of highly complex organic molecules comes from the infrared observations of galactic sources. Figure 1 gives a comparison of calculation and observation for the source Becklin-Neugebauer (BN). The calculation was made by Professor Wickramasinghe of University College Cardiff and myself, for a solid particulate material with the transmittance properties shown in Fig. 2. The solid in this Figure is the measured transmittance of cellulose, and this was followed in the calculations except for two limited wavelength ranges, one near 3 μm, the other near 11 μm, where the broken curve of Fig. 2 was used. The small modification near 3 μm was made (about a 20% decrease of opacity) because free water was present in the measured cellulose samples, and this free water added unwanted opacity in this region. We also increased the opacity near 11 μm by about 50%, because we think some allowance should be made for a hydrocarbon component within the interstellar material, and it is near 11 μm that the hydrocarbons of interest have by far their strongest absorption band. However, this modification affects only a small part of the spectrum over which such calculations can be made. Thus, Fig. 3 is another calculation, extending from ∿ 2 μm to ∿ 30 μm for the source OH 26.5+0.6, and our only deviation from the cellulose transmittance constitutes only a detail in the right-hand-side of the minimum at 9.5 μm.

A mistaken view is quite widespread that the infrared observations can be explained equally well by magnesium silicate particles, but such particles do nothing shortward of ∿ 9 μm, and they also fail longward of ∿ 20 μm. So the contribution of such particles would at best be confined to a narrow wavelength range of Fig. 3. Moreover, the maximum quantity of magnesium silicate (consistent with the known density of interstellar material) falls short of the amount of particulate material required to explain the extinction of starlight (which we can see in Fig. 4) by a considerable factor. Only the C, N, O, group of elements, in combination with each other and with H, are abundant enough to provide a sufficient density of solid particles; and indeed, from ultraviolet observations we know that C is very deficient in the gas phase - so C must of necessity be largely present in the solid phase.

Table 1. Observed Interstellar Molecules in Dense Molecular Clouds

Inorganic		Organic
	Diatomic	
H_2 hydrogen		CH methylidyne
OH hydroxyl		CH^+ methylidyne ion
SiO silicon monoxide		CH cyanogen
SiS silicon sulfide		CO carbon monoxide
NS nitrogen sulfide		CS carbon monosulfide
SO sulfur monoxide		
	Triatomic	
H_2O water		CCH ethynal
N_2H^+		HCN hydrogen cyanide
H_2S hydrogen sulfide		HNC hydrogen isocyanide
SO_2 sulfur dioxide		HCO^+ formyl ion
		HCO formyl
		OCS carbonyl sulfide
		HNO nitroxyl
	4-Atomic	
NH_3 ammonia		H_2CO formaldehyde
		HNCO isocynanic acid
		H_2CS thioformaldehyde
		C_3N cyanoethynyl
	5-Atomic	
		H_2CHN methanimine
		H_2NCN cyanamide
		HCOOH formic acid
		HC_3N cyanoacetylene
		H_2C_2O ketan
	6-Atomic	
		CH_3OH methanol
		CH_3CN cyanomethane
		$HCONH_2$ formamide
	7-Atomic	
		CH_3NH_2 methylamine
		CH_3C_2H methylacetylene
		$HCOCH_3$ acetaldehyde
		H_2CCHCN vinyl cyanide
		HC_5N cyanodiacetylene
	8-Atomic	
		$HCOOCH_3$ methyl formate
	9-Atomic	
		$(CH_3)_2O$ dimethyl formate
		C_2H_5OH ethanol
		HC_7N cyanotriacetylene
		C_2H_5CN ethyl cyanide
	11-Atomic	
		HC_9N cyano-octa-tetrayne

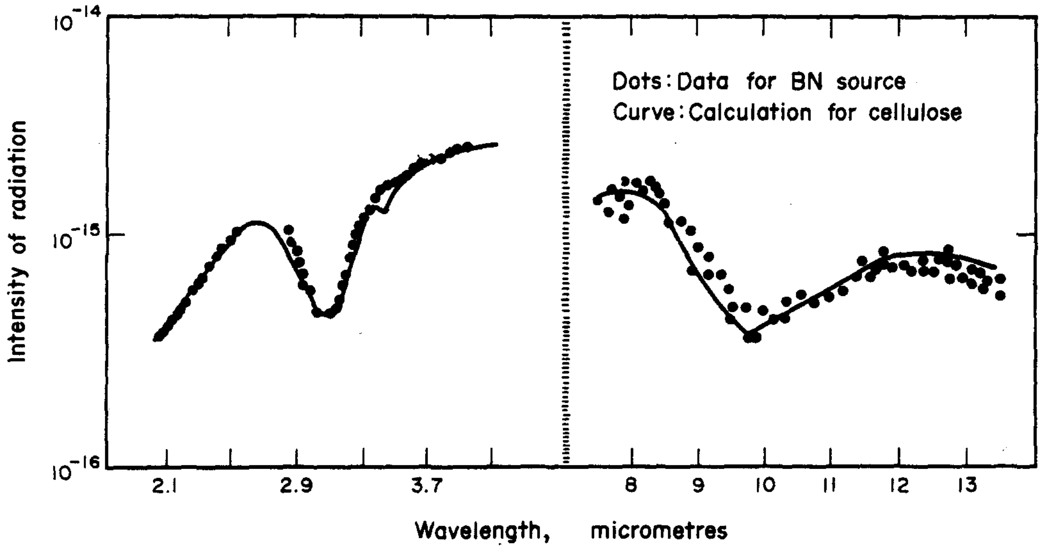

Fig. 1. Infra-red spectrum of the astronomical source BN (Becklin, Neugebauer) compared with predictions for a cellulose model.

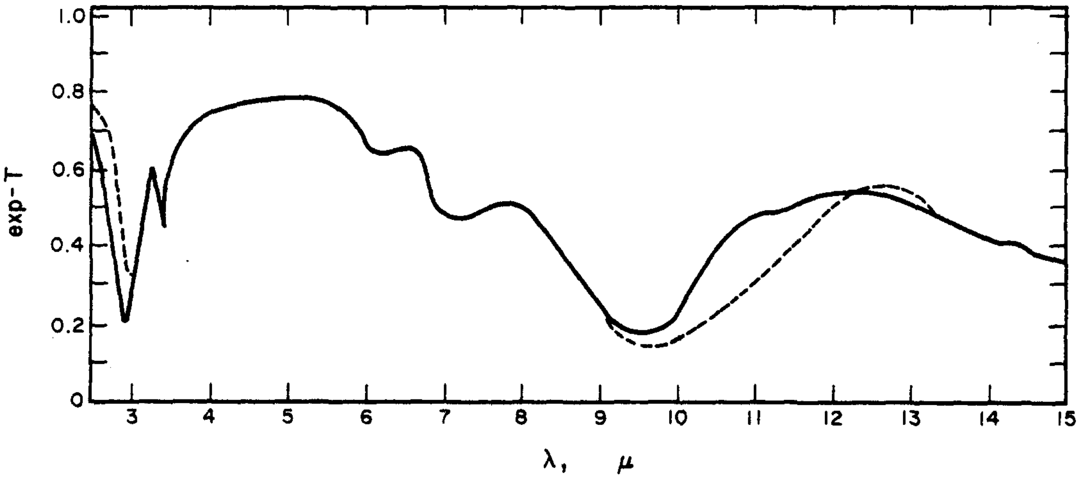

Fig. 2. Transmittance curve for cellulose (solid curve) modified (dashed segments) to take account of dehydration and hydrocarbon impurity.

Returning now to the Earth-bound theory of the origin of life, this theory starts with reduced *in*organics, H_2O, CH_4, NH_3, HCN, ..., which are then supposed to have been processed into organic by solar ultraviolet light and by energizing events like thunderstorms. In these days, when we are well-used to calculating complex networks for chemical (or for nuclear) reactions, it is no surprise to find that in the recombination of fragments from the wholesale break-up of such organics a small trickle of organic molecules is produced. But to chemists and biologists in the 1950s, when experiments to this effect were first put ~~forward~~, the result came indeed as a surprise, a surprise that had led many to believe that such energizing processes could be a satisfactory mode of origin for the organic molecules which formed the basis of life. The trouble, however, was that a *large* input energy was needed in the experiments to produce a *small* output of organics. The poor yield implies that only minuscule quantities of organics could have been produced on the primitive Earth, and of course minuscule quantities would readily have been swallowed up into the terrestrial oceans. It has been suggested that evaporation of water from ponds and lakes and perhaps at the margin of the sea, might have produced locally-enhanced concentrations of organic materials. But such enhancement would necessarily have been short-lived, and biochemistry would have been hard put to it to achieve all the complex transformations needed for the origin of life during only the few months or few years for which any such evaporates persisted in viable forms.

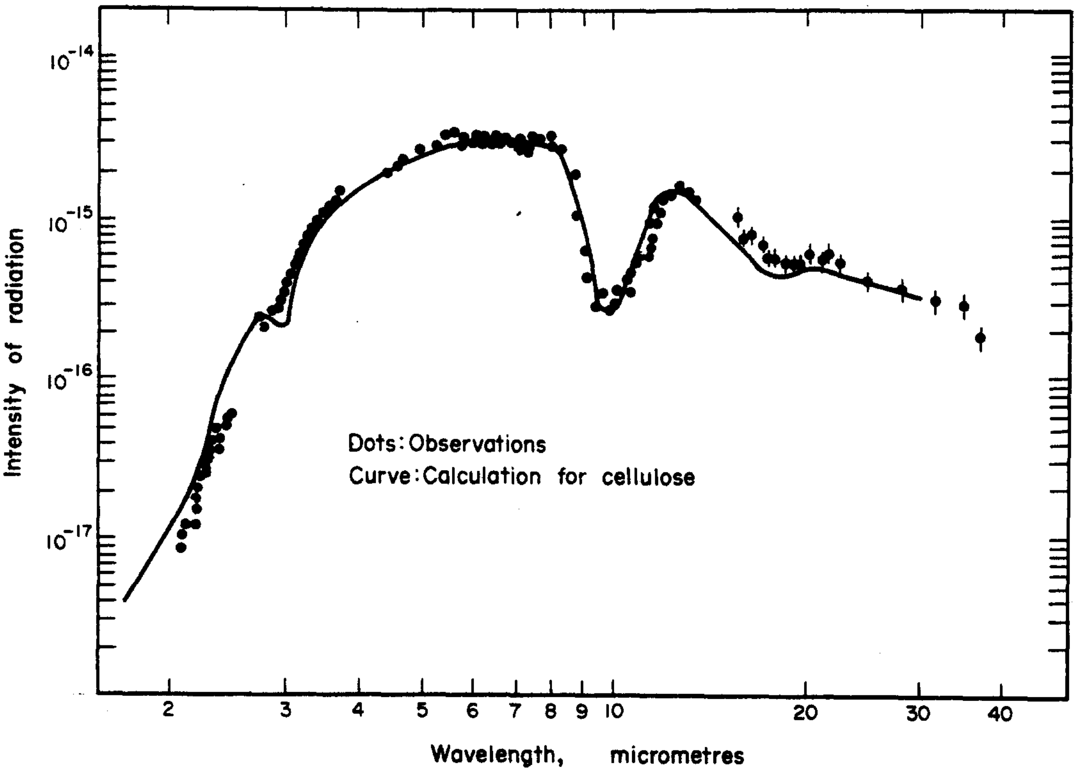

Fig. 3. Infra-red spectrum of the astronomical source OH26.5+0.6 compared with predictions for a cellulose dust model.

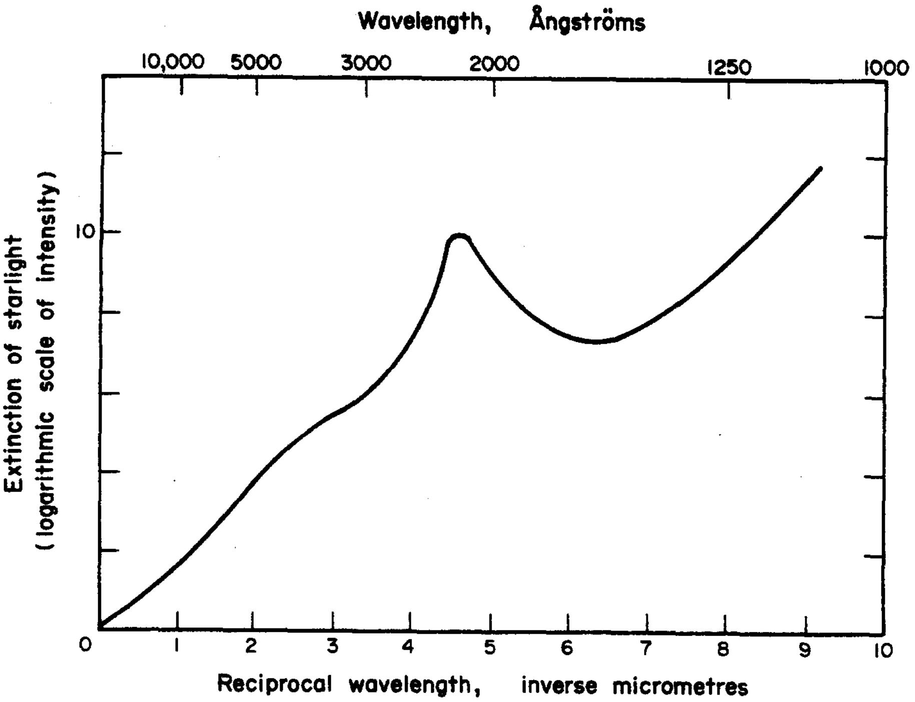

Fig. 4. Dimming (extinction) of starlight in its passage through cosmic dust clouds.

More critical still for the Earth-bound theory is its requirement for the atmosphere to have been non-oxidizing. If initially there were no free oxygen, and consequently no atmospheric ozone layer, the dominant effect of solar ultraviolet light would be to photodissociate water, not to produce a trickle of organic molecules. Then the free oxygen and hydrogen coming from the dissociation of water would be automatically separated by the planetary gravitational field, with the hydrogen escaping quickly into space to leave behind a large unwanted excess of oxygen. An attempt to escape from this simple but serious difficulty has been made by postulating the existence of an initial large store of free atmospheric hydrogen, a store large enough for some hydrogen to persist for long enough to permit life to begin. But the very low concentrations of the rare gases Ne, Kr, Xe, now found in the atmosphere give decisive evidence that there was no such initial large store of hydrogen, for we can scarcely suppose that there would be hydrogen without the normal solar system proportions of the rare gases also being present. Nor can we reasonably argue that the rare gases somehow contrived to escape away into space, because Kr, Xe certainly, and Ne very probably, have atomic weights that are far too high for them ever to have been lost in this way.

Some scientists (who in spite of these arguments would still like to continue with the idea that life evolved from an initial supply of inorganics sited here on the Earth) have argued that interstellar organics added to the solar system would all have been disrupted by the early high temperature of the solar nebula. This rather desperate attempt to avoid the obvious is not all well-founded, however. Even if we grant that all the organics initially present in the solar nebula would have been so destroyed, the early high-temperature phase of the solar nebula was very short-lived ($<\sim 10^5$ years), and on a time-scale much longer than this the solar system must have *continued* to acquire organics from the particular interstellar cloud in which our system was born. This we can see by noticing that a stage in the development of the planets must have been reached when the precursor material of Uranus and Neptune moved around the primitive Sun at a radial heliocentric distance of some 20-30 A.U.

This precursor material of Uranus and Neptune must have contained at least 1500 Earth masses of H and He, since this amount of H and He would have been required to make up the normal solar proportions of these gases in relation to the C, N, O now present in Uranus and Neptune. So we have to think of the solar system as possessing a disk of gas of quite large radius ($\sim$25 A.U.), and of it being immersed still within an interstellar cloud of quite high density - namely, the cloud from which our system was born. Motion at a speed of a few kilometers per second between the solar system and the cloud would be inevitable, and the motion would lead to a contact-sweeping of cloud material onto the planetary disk of H, He, a process with two effects: one, the addition of organics from the interstellar cloud, the other a gradual evaporation of H, He from the periphery of the solar system. For H, He to evaporate, a temperature generated by friction of only a few hundred degrees K would suffice, much less than the temperatures that would disrupt or evaporate the organics themselves.

This process of contact-sweeping could have lasted for upwards of 10^7 years, during which time interval the mass of interstellar gas encountered by our system could well have been of the order of 1000 Earth masses. Indeed, to evaporate 1500 Earth masses of H, He from the region of Uranus and Neptune, the external cloud material encountered by the planetary disk would need to be of this order, and contained within 1000 Earth masses of cloud material there could well have been upwards of 1 Earth mass of organics, a vast quantity compared to any minuscule amounts that could ever have been generated *in situ* at the surface of the Earth.

Although the gas density within the planetary disk ($\sim 10^{-12}$ gm cm^{-3}, before the H, He were evaporated away) was considerably higher than the density of the interstellar cloud ($\sim 10^{-20}$ gm cm^{-3}), it was yet small compared to the densities of gases studied in the laboratory, and this ensured that incoming interstellar organics were not disrupted collisionally. The small solid particles responsible for the scattering of starlight (Fig. 4), and which seem to consist largely of organic polymers (Figs 1, 2, 3), would be slowed gently in the manner of the present-day micrometeorites that are added constantly to the terrestrial atmosphere. And at the low temperature ($\sim$100 K) at the periphery of the solar system, the material would not be subjected to chemical disruption. And in those regions, the regions of Uranus and Neptune, H_2O from the solar nebula had already condensed into a multitude of icy planetesimals, and it would be on top of those planetesimals that the interstellar organics would eventually be deposited.

The time-scale for the formation of Uranus and Neptune was long; just how long is a matter of debate. My own estimate is $\sim$300-500 million years, about one tenth of the present age of the solar system, a longer time-scale than for the condensation of Jupiter and Saturn, and much longer than the condensation time for the Earth. So the planetesimals in the region of Uranus and Neptune went on existing as a huge swarm for hundreds of millions of years after the Earth itself had formed. If the planetesimals had masses similar to present-day comets, say $\sim 10^{17}$ gm, there must have been $\sim 10^{12}$ of them. While some probably had masses much above 10^{17} gm, the swarm of planetesimals was quite certainly enormous. It was composed of bodies with icy cores of radii $\sim$10 km or more; and because of the contact-sweeping of organics from the surrounding interstellar cloud the bodies acquired outer shells of organic material with thickness of $\sim$1 km or more.

What, we may next ask, would be the physical and chemical evolution of such bodies? The main physical evolution lay in a gradual agglomeration into the larger bodies that went to form the planets Uranus and Neptune. Before Uranus and Neptune became fully condensed, however, a fraction of the planetestimals would be sprayed into a giant halo surrounding the solar system. This would arise inevitably from sequences of dynamical encounters between a fraction of the planetesimals and the forming planets themselves. Some bodies would go into orbits that permitted them to survive from being added to Uranus and Neptune for longer than others, and some may well have survived to the present day. Indeed, gravitational perturbations from the nearest stars would sometimes cause the orbits of distantly-sprayed planetesimals to acquire perihelion distances $>\sim$100 A.U., and such bodies would then form long-term attachments to the solar system.

Next, let us think of the chemical evolution of the planetesimals with their icy cores and organic skins. Chemical energy was to be had from the organics, whether from individual molecules like those of Table 1 or from organic polymers like cellulose. Under hard-frozen conditions at $\sim$100 K, the chemical energy would not be subject to spontaneous release, however, but from time to time there would be transient events, as for instance if a smaller body should hit a larger one, that would set up a chemical detonation. Thermal conductivities were low, so that heat released within the bodies would remain trapped there, permitting a melting of ice near its boundary with the organic material. A further release of chemical energy on a long time-scale, as for instance through glycosis, would continue to heat the mixtures of organics and water, leading to a situation analogous to Charles Darwin's postulate of a "warm little pond":

> " . . . if (and oh what a big if) we could conceive in some warm little pond, with all sorts of ammonia and phosphoric salts present, that a protein compound was chemically formed ready to undergo still more complex changes . . . "

Except that instead of just one warm little pond we now might have warm little ponds for thousands of millions of the planetesimals, ponds that remained warm, not for a few months only, but for millions of years. Except also that instead of being supplied with ammonia and phosphoric salts our ponds are supplied with highly complex organics. And except too that instead of being exposed to an oxidizing atmosphere our ponds are safely buried hundreds of metres below protective outer skins.

Such warm cometary ponds quite evidently have impressive advantages over the Earth as sites for the origin of life. The higher concentrations of organic molecules, the enormous profusi of possible objects, the long time-scale, all point toward the region of Uranus and Neptune as the original home of life. Indeed, the description of a "warm little pond" is a little too cosy to give us a proper terrestrial analogue to the real thing. Rather should we think of a vast laboratory with a floor area of some hundreds of square kilometres, with a height greate than the London Post Office Tower, a laboratory chock-a-block with a high concentration in warm water of organic molecules and biopolymers (cellulose perhaps and other sugar chains), a laboratory shielded from damaging radiations, a laboratory with accurate temperature control that went on with its chemical transformations for millions of years. It was in such vast laboratories, $\sim 10^9$ or more of them, that I will suppose life to have begun.

Next, we may wonder how life, having originated in such remarkable cosmic laboratories, came t make a transition to an eventual home for itself here on the Earth. For this problem, let us go back to the gravitational perturbations which caused some of the bodies to acquire unusual orbits, orbits which enabled them to escape from being swept-up into Uranus and Neptune. From time to time, other gravitational perturbations must have caused a fraction of the bodies to come close the the Sun, instead of being sprayed out and away from the Sun. A fraction of these bodies would approach to heliocentric distances ~ 1 A.U., as happens even today for a few comets each year. During the first few hundred million years of the history of the Earth, before Uranus and Neptune were fully formed, planetesimals must have been approaching to within ~ 1 A.U. much more frequently than comets do nowadays. At each perihelion passage, a part of the outer organic skin would have been peeled away from the surfaces of the bodies, bringing any life that had evolved inside them closer and closer to the outside world. The peeling-away process could have been due to a simple thermal evaporation, although I think it likely that in some cases the removal of the outer skin would be assisted by the release of chemical energy still contained within the organic material. Surrounding water would freeze into ice, temporarily immobilizing life processes. Then, with sufficient evaporation, the living cells would at last come free, perhaps to be quickly destroyed by sunlight, perhaps to be shielded by inert solid material, perhaps even to survive in the presence of sunlight. If the Earth happened to be nearby, such a cell - or perhaps many cells - would become entangl in the terrestrial atmosphere. Some would be slowed gently and survive their slow fall down to the Earth's surface, where they might well find that a supply of the chemical foods to which they were accustomed had already arrived ahead of them. Survival until the chemical foods became exhausted would then be a possibility, but not indefinite survival unless a new trick could be learned.

Life is known to have passed from anaerobic to aerobic conditions, and there has been much controversy over why, how, and when this transition took place. On the present view there is scarcely any alternative to fixing the transition at the moment of transfer from cometary object to Earth. But survival on the Earth now depended on the trick of photosynthesis. This trick must have been learned before the Earth's indigenous supply of chemical foods ran out, or before a continuing rain of foods from cometary objects to the Earth fell away to an insubstantial trickle. Photosynthesis recovered the initial situation, however, by enabling the supply of foods to be constantly regenerated *in situ*. With this crucial step, life may be said to have made the uneasy transition from its cometary home to the Earth.

What, we may ask, are the further consequences of this picture? The picture requires that life arose frequently inside the cometary bodies. If it happened inside only one body the chance of that particular body transferring living cells to the Earth would be very small; and even if cells were transferred, the chance of those particular cells happening to solve the photosynthesis problem would probably also be small. But if many cometary objects each possess a profusion of living cells the situation would be otherwise. Sooner or later one or other among them would shed cells onto the Earth, and if on the first occasion when successful shedding occurred the photosynthesis problem were not solved, further occasions would be coming along. If one is to have the present picture, this is the way it must surely have been. The picture is strikingly similar to the scattering of seeds in the wind. Few are destined to take root, but so many are the seeds that some among them manage to do so.

Long-term survivors from the vast number ($\sim 10^{12}$) of early planetesimals may still approach the Earth today. So we are led inevitably to consider the further question of whether such objects still continue to shed living cells (or highly advanced biochemical systems) onto the Earth, and if so do we have evidence of such visitations? Following the logic I have so far described, I feel that we are driven to an affirmative answer to the first of these questions, and in an attempt to answer the second question, Professor Wickramasinghe and I were recently led to consider whether cometary material might even today be bringing new viruses and bacteria to the Earth.

Comets were feared by many ancient cultures as the bringers of pestilence and death. We ourselves, living in the century following Pasteur, have grown up to think of this old belief as a ridiculous superstition, and many biologists will no doubt question the wisdom of reviving it at the present juncture. But the usual view, that bacteria and viruses are all indigenous to the Earth, rests for its security on the assumption that life itself originated on the Earth. Once that assumption is threatened, as I think it certainly is threatened, all that rests thereon becomes exposed to doubt.

So let us proceed with the hypothesis that living cells and viruses still reach the Earth. First, let us note that the Earth is known actually to sweep-up particles with the right range of sizes, from aerosol particles with sizes of ~ 100 Å up to micrometeors with sizes of ~ 10 μm or more. These particles are thought to be cometary in origin, and the larger particles (~ 3 μm) are known to contain carbon, oxygen and nitrogen, the carbon not fixed, but in a volatile form. Next let us note that compared to the amounts of virus or small bacteria that would be of relevance to the diseases of terrestrial planets and animals, the amount of the particles actually entering the upper atmosphere is very large. The annual infall rate for the whole Earth has been estimated at about 20,000 tons. If 1 part in 10^6 of that amount were viruses, there would be of the order of 10^{22} virus particles added each year, more than 10^7 per year to each square metre of the Earth's surface.

Virus particles (or bacteria) could be shielded from sunlight by being embedded in a protecti matrix of solid material. Cellulose, the material I discussed earlier in connection with the infrared emission of galactic sources, would remain very cool, because cellulose is an exceedingly poor absorber of visual light, but a good radiator of heat. The temperatures of virus particles embedded in cellulose (and in certain other organic materials), would thus be much below the so-called "black sphere" temperature. Near the Earth, the temperature would be ∿150 K, providing a hard freeze-dried condition for virus particles.

Nor would particles with sizes up to ∿10 μm be seriously heated on entering the diffuse upper atmosphere of the Earth. They would be slowed gently to be carried eventually to the ground, where they could encounter plants and animals or where they could become incorporated into terrestrial soil and dust. The time of fall of a small particle through the stratosphere is quite sensitively dependent on its size. Those with dimensions ∿3 μm take but a few months to reach the tropopause, whereas those of ∿0.3 μm take several decades. Still smaller particles, <∿0.1 μm, may be speeded up again by electrical fields that are generated through the incidence onto the Earth of particle streams from the Sun.

Once below the stratosphere, particulate material would follow air movements, and such movements are of course complex, being subject both to continental and local meteorological variations. These could, moreover, be variations in the manner of addition of pathogens at the top of the atmosphere. Some samples could be geographically patchy, perhaps even on a horizontal scale of but a few kilometres, while other samples could be incident over individua nations, or over continents, or even more-or-less uniformly incident over the whole Earth. Very marked fluctuations in the arrival of pathogens are therefore to be expected, in qualitative correspondence with the complex (and seemingly bewildering) outbreaks of some epidemic diseases among plants and animals.

While cometary debris in the neighbourhood of the Earth is not derived from a single comet but from several, particular comets make the major contribution to the debris over portions of the Earth's annual orbit around the Sun. Thus, since pathogenic material from different comets would be expected to vary in kind, it is to be expected that such material incident on the Earth would change both with respect to the Earth's position in its orbit, and on a longer time-scale as debris arrived from new comets. The pathogenic attack on the Earth would therefore be perpetually varying, in weeks or months on a short time-scale, in decades, centuries and millennia on a longer time-scale.

From what I have just said it would be expected that the attack of diseases resulting from the incidence of pathogens from outside the Earth would be considerably variable, both with respect to clumpiness and with respect to the moments of first penetration of a new pathogen down through the atmosphere. There would be cases of more-or-less simultaneous "strikes" at widely separated geographical locations, giving the impression of enormously high transmission speeds, if interpreted as a creature-to-creature spreading.

The expected situation is well-illustrated by the following extract from an article by Dr. Louis Weinstein entitled "Influenza - 1918, A Revisit" (*New England Journal of Medicine*, May 6, 1976):

> "The influenza pandemic of 1918 occurred in three waves. The first appeared in the winter and spring of 1917-18 ... This wave was characterized by high attack rates (50 percent of the world's popula-

> tion was affected) but by very low fatality rates ... The lethal second wave, which started at Fort Devens in Ayer, Massachusetts, on September 12, 1918*, involved almost the entire world over a very short time ...
>
> Its epidemiologic behaviour was most unusual. Although person-to-person spread occurred in local areas, the disease appeared on the same day in widely separated parts of the world on the one hand, but, on the other, took days to weeks to spread relatively short distances. It was detected in Boston and Bombay on the same day but took three weeks before it reached New York City, despite the fact that there was considerable travel between the two cities. It was present for the first time at Joliet in the State of Illinois four weeks after it was first detected in Chicago, the distance between those areas being only 38 miles ..."

Dr. Weinstein used the phrase "person-to-person", and doubtless such spread occurred, but really firm evidence for the person-to-person transmission of influenza is hard to come by, at any rate among major population groups. I would say that person-to-person spread at an average rate of ∿1 mile per day might have explained the transmission in a month from Chicago to Joilet, but a person-to-person spread within only a day or two from Massachusetts to India was not possible in 1918, before air travel.

Professor Magrassi in Sardinia had this to say about the lethal second wave of the 1918 pandemic:

> "We were able to verify ... the appearance of influenza in shepherds who were living for a long time alone, in solitary open country far from any inhabited centre; this occurred absolutely contemporaneously with the appearance of influenza in the nearest inhabited centres."

The spread within weeks in the autumn of 1918 to remote villages in Alaska was said to have been occasioned by boats, but Governor Riggs had this to say to the U.S. Senate Committee of Appropriations (January 6, 1919):

> "At Kodiak, which is an island in Western Alaska off toward the Alentian Islands, they had some 300 cases of influenza, and owing to the fact that mail contracts had been cancelled there were *no* boats ... "

The lethal second wave of the 1918 pandemic also gave evidence of local patchiness, well-nigh impossible, I would think, to explain in terms of person-to-person transmission. Death-rates from respiratory diseases recorded in the last months of 1918 were remarkably variable from one American city to another. A striking contrast came from Pittsburg and Toledo, neighbouring cities with normally almost identical death-rates, and with populations of similar age-grouping that are concerned in similar daily occupations. The late 1918 death-rate from respiratory disease in Pittsburg exceeded that in Toledo, not by a modest few percent, or even by a few tens of percent, but by an enormous 400 percent.

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*In another reference in the literature, the date is given as August 12, 1918, for the outbreak in Massachusetts.

To the surprise of epidemiologists, air travel has not brought an increase in the rate of spread of influenza. The 1968 influenza pandemic was not as spectacular as that of 1918, but it was more precisely documented. Figure 5 shows the indicence in 1968 of influenza outbreaks in the U.S.A., given state-by-state over a period of 12 weeks. The first outbreak of the 1968 strain of the influenza virus had already occurred in Hong Kong. Because California is the nearest U.S. state to Hong Kong, it might seem natural that the first U.S. outbreak occurring in weeks 40-42 should have been in California. But it is decidedly less natural that the first California outbreak should have appeared in the small remote town of Needles, not in the high population centres of Los Angeles, San Diego, or San Francisco. It took some 3 weeks for the disease to spread into the contiguous states of Nevada, Arizona and Colorado. By that time, it had also made a leap, not along the popular air routes to New York or Chicago, but to Montana. Week 46 shows a further contiguous filling-in among the mountain states; and at last, some 6 weeks from the first outbreak, there is a leap to New York, but still not to Chicago, or to Dallas or to Miami. By week 47 a patch in the north-east is filling out, Chicago has it, and Texas has joined the west. Week 49 is interesting for the circumstance that the well-known intensive November-December holiday traffic from the north-east to Florida has still not infected Miami. While boats might carry influenza with uncanny speed, planes clearly do not do so.

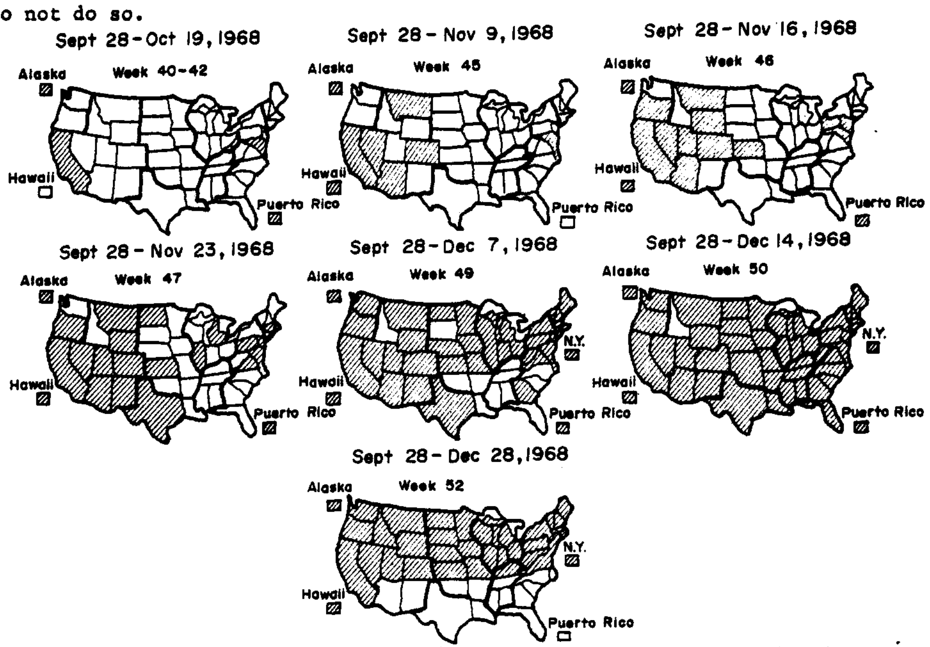

Fig. 5. State-by-state spread of influenza during the 1968 epidemic (from *Influenza - Virus, Vaccines, Strategy* (Ed. P. Selby) Academic Press, 1976).

The pattern of Fig. 5 is plainly geographic. The disease hit first at the Pacific and mountain states. The next focus hit the north-east and the Great Lakes area. The last to be reached was the south-east. All this is consistent with an infall of the influenza virus from space, and with atmospheric patterns causing geographic variations in the time of infall.

How could an Earth-bound theory cope with facts like these? Not through the old idea of a genetic mutation of the influenza virus followed by a simple person-to-person spreading. Besides the difficulties I have mentioned, there are difficulties of a technical nature facing this idea. So other possibilities are at present under discussion, which have been suggested by the discovery of the presence of influenza virus in pigs, horses and birds. A hybrid genetic cross, of the kind first shown by Macfarlane Burnet to be possible in the laboratory, is postulated to occur under natural conditions, the cross taking place between a human virus and an animal virus. A subclinical spread of the new recombinant virus is postulated next. If the subclinical spread occurs within the animal population, then the animal might carry the virus all over the world, as for instance birds might do, shedding the new virus everywhere - in bird muck perhaps - and so to be picked up everywhere by humans. Or if the new virus spreads subclinically within the human population itself, the virus might be "shocked" into virulence by some natural event - by the weather or by an earthquake.

Perhaps because I came to this recent development of the Earth-bound theory only after I had begun considering the cometary theory, I find the postulates I have just outlined to be quite "far out". To me it smacks of absurdity to argue that a storm made the difference between Pittsburg and Toledo in 1918 (there was no suitable storm), and I am left wondering what manner of bird would be flying in October to the Aleutian Islands. Birds fly south at that time of the year. And to me it seems entirely natural that other animals should tend to be attacked by the same invading pathogens as ourselves, and for respiratory diseases birds are especially vulnerable to attack, because they sample the air more widely that do land-based animals - if the attack is patchy, powerfully-flying birds will inevitably find the pathogenic patches whereas land animals may often escape them.

There are, of course, many diseases where a pathogenic reservoir has become terrestrially established, and where a reservoir in one animal may serve to infect another, different species. But the epidemiology of such diseases, once the reservoir and its carrier have been identified, is always quite obvious. Such interchange-diseases may well also have arisen in the first place from space, but with a reservoir once established further arrivals from space may become less important as a source of pathogenic material than the established reservoir itself. With influenza in humans the situation appears to be otherwise, however. Influenza appears to exist in humans only in so far as it is continually driven from outside. It is this driving from outside which explains the strange epidemiology of influenza, and which also explains the fact that with the arrival of a new system of the influenza virus the previous subtype is found to disappear. On the cometary hypothesis, this otherwise mysterious fact becomes the mere trivium that if something changes it changes.

Even if a space-incident pathogen should succeed in establishing an Earth-bound reservoir for itself, long-term survival for the pathogen is by no means guaranteed. Ecological shifts, climatic variations, and the development of immunity within the host plant or animal are all ever-continuing threats to the survival of the pathogen. One can therefore wonder what history has to tell us about the continuity or otherwise of human diseases.

From their nature one might expect viruses to be more ephemeral than bacteria. In correspondence with this expectation the bacillus-caused diseases of pneumonia and tuberculosis are quite recognizable in the books of Hippocrates (5th century B.C.), whereas unequivocably recognizable descriptions of the virulent viral diseases are rare. This has been put down to overwriting by later doctors, who might have misunderstood which particular diseases Hippocrates was describing. On the other hand, an extensive account of an exceedingly virulent disease which hit Athens in 429 B.C., and which lasted thereafter for 4 years, was given by Thucydides in his *History of the Peloponnesian War*, and this account has certainly not been subjected to overwriting by later authors. Although it is possible to pick out subsets of the symptoms described by Thucydides that can be fitted to modern diseases, no modern disease fits at all plausibly to the whole of the symptoms. This difficulty for the conventional view of the nature of infectious diseases has sometimes been countered with the suggestion that Thucydides exaggerated his description, but this suggestion does not square with either the historical precision of the rest of his book or with the care that Thucydides took to use the exact medical terminology of his day. There are few, if any, witnesses in ancient history that have been judged by clinical scholars to be more trustworthy than Thucydides.* So any view of the nature of infectious diseases which suggested otherwise must itself be exposed to doubt.

The likelihood is that what Thucydides said was exactly true, in which case a disease struck Athens like a lightning bolt in 429 B.C. - a disease which has no modern counterpart.

If ever there were a disease which we might have expected on the usual point of view to have had widespread persistence throughout history, it would be smallpox. Smallpox is highly infectious, and each new generation of children provide a supply of persons without immunity to the disease. Yet smallpox did not appear in a certainly recognizable form until the 7th century A.D. It may have existed in Egypt (and possibly also in India and China) some 2000 years earlier than this, around 1200 B.C. Some commentators have also suggested that the so-called "plague of Galen" in the 2nd century A.D. might have been smallpox, although this seems rather doubtful in view of the etymology of the Latin word for pock mark, the word *Variola*, which was not coined until the 7th century A.D. The need to coin the word at such a late date implies that no older word existed, implying also that the disease itself did not exist in classical Rome.

Either smallpox occurred sporadically in history, with many centuries intervening between successive epidemic outbreaks of the disease, or we must take the view that the disease appeared mysteriously for the first time in Europe in the 7th century A.D. It is usually said to have arrived from the "east", although one is left to wonder how a weakly-transmissible disease like influenza could travel almost instantaneously around the whole world in 1918, whereas a truly infectious disease like smallpox required many centuries, if not indeed millennia, to spread into Europe from its presumed focus in the east - in spite of the wide-ranging forays of Greek, Persian and Roman armies.

The historical evidence thus suggests, even if it does not rigorously prove, a pattern of changing viral disease, changing for virulent diseases on a time-scale of the order of 1000 years. Viral pathogens that have been with us for significantly longer than this have become more accommodated to their hosts. Smouldering diseases are caused by longer-term invaders, not deadly any more, but not entirely assimilated either. The virus *herpes.simplex* (which causes cold sores) is an example of such a smouldering longer-term invader.

In my last remarks I wish to turn to a deeper logical problem. Is it not remarkable, several microbiologists have asked, that extraterrestrial pathogens should possess the ability to attack highly-evolved terrestrial life-forms? I must agree that it is so remarkable that only a far-reaching explanation can be judged satisfactory, but before I suggest what the explanation might be I would like to turn the question around, from a defensive to an attacking posture. Is it not remarkable within Earth-bound theories to have to admit that complex terrestrial life-forms have failed during their long evolution to develop a total immunity to all infectious diseases?

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*E. Watson Williams, *Greece and Rome*, IV, No. 1 (1957).

One microbiologist, conscious no doubt of the possibility of the tables being turned on him in this fashion, remarked that there is a sense in which pathogens can "think", as if to say that they possess the ability to devise a new trick to overcome every immunological device with their hosts may adopt. But of course bacteria and viruses posses no such literal ability. They can only evolve in a Darwinian sense in relation to the immunities developed by their hosts. If a host should happen on a form of immunity for which no answering mutation were in existence, there is no intellectual sense in which a pathogen could set about inventing such a mutation. Pathogens must always have already in hand mutations for dealing with the immunological shifts of their hosts, otherwise they become extinct. Pathogens were thus balanced on a razor-edge between survival and extinction, a balance which seems to me to be more acceptable if it has only to be maintained on time-scales of centuries or millennia, rather than on the much longer time-scale that of necessity one must have in the Earth-bound theory.

Virulent viruses are particularly exposed to extinction. There are four steps in the complex process whereby such a virus multiplies itself, the preventing of any one of which confer immunity on an evolved plant or animal. The virus must first have an attachment protein and an attachment site to a host cell. Second, the interaction of the attachment protein to the cell wall must serve to strip other viral proteins away from the genetic material of the virus, which must then be afforded naked ingress to the host cell. Third, the viral genetic material must have the ability to overwrite the normal genetic program of the cell. And fourth, after multiplying itself, the new viral particles must be able to gain egress from the host cell in order to attack new cells.

With so many opportunities to frustrate the attack of viruses, I find myself asking why evolved life-forms do not avail themselves *convincingly* of these opportunities. Immunity, such as it is, consists mostly of preventing the entry of virus particles, and then only the entry of specific viruses, not of viruses in general. It would be far more effective for host cells to develop genetically so as to prevent overwriting by viruses in general. Logically such a possibility must exist, because the greater quantity of information present in the genetic material of the host must be able to overcome the lesser quantity of information in the virus, or indeed to overcome the combined information of many viruses. Yet such a defence is never presented, except possibly very transiently during the course of an infection, when a substance called "interferon" may have some such effect. Rather does it seem that host cells deliberately avoid the seemingly best form of defence. The situation is not at all that the virus is "clever", but that the host appears to be incorrigably "stupid". Indeed, "stupid" may not be a strong enough word. "Deliberate suicide" would seem nearer the mark, since host cells invite viral invasions by apparently deliberately providing sites to which viruses can attach themselves*.

Let us see, by way of concluding this discussion, if we can track down what the reason might be for this seemingly astonishing behaviour. Complex life-forms have increased the number of their genes at an average rate of about 1 per 1000 years; and in periods of rapid evolution, as in the emergence of mammals following the cretaceous, the gene acquisition rate was probably appreciably higher than the average. Where, one can wonder, do new genes come from? Not from

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*I find a number of points to have been made by N. G. Anderson, *Nature*, 227, 1347 (1970).

simply dividing existing genes, since divisions would degrade information instead of increasin it. And the changing of codons within existing genes hardly seems adequate either.

A process of gene addition does exist, however. Viruses have the well-proven ability in some cases of not destroying host cells but of developing a symbiotic relation with them. In some such cases the viral genetic material actually adds to the host genetic material, contributing several new genes at one full swoop. If the evolution of terrestrial life-forms depends for its new genes on this known process, then we can immediately see why a vigorous immunity to viral attack is not possible. Any life-form which developed such immunity would be unable to evolve, except through comparatively minor codon changes of its existing genes. Failure to adapt to an ever-changing environment would soon render such a life-form extinct.

New viral genes would not immediately be "useful". From the point of view of the host cell they would code only for "nonsense" proteins. While "nonsense" proteins could conceivably be changed to "useful" ones through many mutations of the codons of the new genes, a more powerful and quicker way of becoming "useful" would be through a selective reading of the new viral genes. In this connection, it has recently been discovered that messenger RNA often omits whole sequences in the reading of DNA, and this is just the kind of development that would give the most effective conversion of "nonsense" proteins to "useful" ones. Such a selective reading may well be the host cell's final revenge on an invading virus.

Implicit in this last consideration is that our own DNA is by no means the pure genetic material that we usually consider it to be. If literally read it may contain much nonsense. The trick may lie in reading only wanted bits, "wanted" being defined in the Darwinian sense of promoting survival against the background of our environment. In this respect it is interesting that "nonsense" proteins are indeed found to be generated in certain human diseases as if it were the nature of those diseases to promote a literal reading of a portion of our DNA. This observation may be taken as confirmatory of the logic we have been following. I think perhaps I should also mention the astonishing story of what are known as Type C viruses, a story which comes close to being a laboratory demonstration of the line of argument we have been following, a line of argument that begun with organic molecules in space. We asked if there was any way in which organic material from the interstellar clouds could promote life in the solar system. We saw that such material could first be added to the outer regions, the region of the present planets Uranus and Neptune. However, for the first few hundred millions years in the lifetime of the solar system, Uranus and Neptune would still be uncondensed, still existing as a swarm of icy plantetesimals, and it would be on top of the icy plantetesimals that the organic material from interstellar space would eventually be deposited.

Heat from chemical energy released by the organics would be confined for millions of years by the low thermal conductivity of water and of the organic skins of the planetesimals, and ice at depths of several hundred metres inside these would be warmed and melted. This was our organic soup in which we supposed that life began.

Hext, we appealed to gravitational perturbations to bring a fraction of the life-bearing planetesimals into elongated orbits with perihelion distances ∿1 A.U. Repeated approaches to heliocentric distances of this order gradually evaporated the outer layers of the planetesimals, leading at last to the emergence of life from an initially anaerobic world into a sunlit, aerobic world. Probably only a fraction of living cells survived this drastic transition, but so great was their number,so many were the planetesimals, that some cells made the transitions, and some which did so became entangled with the upper atmosphere of the Earth, where they were slowed gently in their motion, to float down at last upon the surface of our planet.

From this "seeds in the wind" picture we were then led to the conjecture that such "seeds" might still be reaching us today. We came, therefore, to look for evidence that this might be so, and in thinking about the pathogenic relation of such "seeds" to life-forms on the Earth we arrived at the radical view that the most profound effect of "seeds" still reaching us could lie in a supply of new genes, and so to continuing evolution towards more complex life-forms here on Earth.

What would such a view do to normal biological concepts? It would make the critical change of denying that gene numbers can be changed in an important way by purely terrestrial processes, a change that bids to clear up a difficulty which for long tormented the great geologist Charles Lyell, a difficulty which even Darwin himself found unanswerable. Lyell believed deeply in the long-term constancy of the terrestrial environment, a belief which modern geology has fully vindicated, taken over the past 1000 million years at any rate. So Lyell argued that if the environment had had long-term constancy, then so must terrestrial life-forms: the one condition must imply the other. Paleontologists of the early 19th century argued that the fossil record showed otherwise, to which Lyell replied that the fossil record - incomplete in his own day - would eventually turn out to support the long-term constancy of biological forms. So far as the vertebrates are concerned, it has not done so, of course, and the evolutionary dilemma seen so clearly by Lyell, and admitted by Darwin, remains with us to this day. What, we may ask, has brought about the undoubtedly increasing complexity of the paleonotological record? Our present answer is that the increasing complexity has come from an increasing aggregate of cometary genes, against which terrestrial natural selection has been able to operate. What we are saying now is that biological evolution, as it is ordinarily understood, is subject to an externally controlled gene number. Terrestrial life-forms are a function of that number, and as the number changes so do the life-forms. Nothing that can happen on the Earth can go outside this externally-imposed constraint, unless in the long-term it should be the self-development of an inorganic computer.

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